

# HUMAN ETHOLOGY NEWSLETTER

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# 31

Univ. of Tenn.  
Knoxville 37916

## NEW EDITOR

Joan Lockard will assume the post as editor of the Human Ethology Newsletter as of December first. Joan has been an active participant in the International Society for Human Ethology since its inception, having served on the Executive Board for the past two years, performed the functions of local host for one of the Animal Behaviour Society meetings, and as contributor and guest editor of Forums in the Newsletter. Any future inquiries or correspondence regarding the Newsletter should be addressed to Joan at Department of Neurological Surgery, School of Medicine, University of Washington, Seattle, Washington 98195. Instructions about preparation of materials and subscriptions appear on the last page of this newsletter.

## FAREWELLS & THANK YOU'S

While editing the Human Ethology Newsletter over the past three years I have received the good assistance of many people that I would like to recognize. Midge Elias served as an enthusiastic book review editor. Jerry Barkow, Derek Freeman, and Wade Mackey deserve special recognition for being the first contributors to the Ethology Forum section of the Newsletter, initiated back in 1977. Joan Lockard was one of the first guest editors of a Forum, the topic was "The Adaptive Significance of Self-Deception." Other Forum guest editors were Glen King, and Marjorie Elias; they were ably assisted in their tasks by the thoughtful colleagues who provided criticism and commentary on various topics. I don't even blush when I say that I know of no other newsletter that has benefitted from such provocative and insightful articles as those contributions. Final recognition must also go to my secretary who has served as typist, layout artist on occasions, and subscriptions manager, Maxine McSwords. A sincere THANK YOU to all.

## ELECTIONS

This issue of the Newsletter contains a ballot for election of members of the Executive Board. Nominations were gathered over the summer and fall and were collected by Joan Lockard. Please read pages three and four for instructions on balloting.

# forum

The window  forum will focus on the topic of "The State of the Science of Human Ethology. The following five questions are posed as points of discussion. Contributions to the forum may address one or all of this questions.

1. Where are we in terms of an integrated discipline?
2. How good and long-lasting are the data we are gathering?
3. Are we in danger of running out of substance?
4. Is the study of human behavior more than the application of the principles of animal behavior to our own species?
5. Where do we now stand in the bigger picture of behavioral ecology?

Anyone may submit a contribution for inclusion in the Forum sections of the Newsletter. Commentary should be limited to five hundred words. DEADLINE for submission of commentary is January 15. Mail contributions to Joan Lockard, her address is listed on the first and last page of the Newsletter.

## NEWS

### NEWSLETTER

Video-Informationen is a new German newsletter concerning itself with the use of video equipment in psychological practice. The newsletter is in German, but includes some reports (stemming from English speaking countries) in their original English text. The newsletter will be publishing reports from video manufactures on specific products (including prices) which have been hand picked by the editorial staff as products they feel will be of interest to the readers. Some reports in the first issue include such topics as film vs. video, ethical problems with filming and video taping, frequent problems in filming and videotaping, and a report from the Institute for Non-verbal Communication. The newsletter will be published three times a year, and issues can be obtained by writing either:

Dr. Heiner Ellgring	or	Harald Wallbott
Max-Planck Institut		Fachbereich Psychologie
für Psychiatrie		Universität Giessen
Kaepelinstrasse 10		Otto-Behaghel-Str. 10 F
D-8000 München		D-6300 Giessen
Fed. Rep. of Germany		Fed. Rep. of Germany

## NEWS (continued)

TV IN PSYCHIATRY is a newsletter edited by L. Tyhurst and published at the University of British Columbia. A survey of recent issues indicates that topics for the newsletter include information on psychotherapy seminars, workshops on simulation activities, audiovisual methods in the study of human social behavior, and announcements of seminars and workshops. The publishing address is TV IN PSYCHIATRY NEWSLETTER Dept. of Psychiatry, University of British Columbia, Vancouver, B.C. Canada V6T 2A1

### ★ Become a Member

The Institute for Nonverbal Communication Research Inc. is a nonprofit, educational organization for researchers, educators, artists, and students interested in nonverbal communication research and its applications. In the past decade the behavioral study of body movement (kinesics, body language, expressive movement) has dramatically increased. The Institute is both figuratively and literally a meeting place for those in diverse disciplines and professions concerned with this fascinating subject. They can meet at the Institute's annual research conference to focus on a key theme in this diverse and wide-ranging area of study or at the Institute's seminars on topics in methodology or applications which are rarely available in universities. They can communicate with each other through The Kinesis Report, news and views of nonverbal communication, published quarterly by Human Sciences Press and sponsored by the Institute. Institute members may also request specific information on current literature and research activity from the Information Service and receive appreciable discounts on Institute publications and important books in nonverbal communication.

# BOOKS

**ETHOLOGY AND NONVERBAL COMMUNICATION IN MENTAL HEALTH**, S.A. Corson & E.O'Leary Corson, Pergamon Press, 1980, 290 pp. \$41.00.

This is an interdisciplinary treatment of ethological and anthropological approaches to mental health. This symposium considers the role of nonverbal communication in the development, diagnosis, treatment and prevention of behavioral and psychophysiological disorders.

**MOTIVATION OF HUMAN AND ANIMAL BEHAVIOR : AN ETHOLOGICAL VIEW**, Konrad Lorenz & Paul Leyhusen. Van Nostrand Reinhold Co., 432 pp. \$22.50.

The authors compare human and mammalian behavior in such areas as expression, social organization, motivation, sex education, fear, and experience and learning.

**GRZIMEK'S ENCYCLOPEDIA OF ETHOLOGY**. Van Nostrand Reinhold, 680 pp. \$39.50.

The book begins with background information on animal systems and sense organs, and goes on to examine areas such as learning and communication behavior, courtship, and pairbonding, orientation in time and space, prenatal and postnatal behavior development, and aggression and stress adaptation.

ELECTION OF EXECUTIVE BOARD MEMBERS

The International Society for Human Ethology has a governing structure of an 8-person executive committee. This executive board is elected by the membership with the PROVISO that the members must include at least one individual from each of the following discipline categories:

Animal Behavior

Anthropology

Psychology

Other Social Science

The composition of the current Board by primary discipline includes

Animal Behavior: Irenaus Eibl-Eibesfeldt  
William C. McGrew

Psychology: William Charlesworth  
Cheryl Travis  
Joan Lockard  
Donald Omark

Anthropology: Glen King

Psychiatry: Ronald Simons

The term of the last four members listed ends December 31, 1980. Four new members are to be elected (see ballot below) whose term will begin January 1, 1981 and run for two consecutive years. Henceforth four new members will be elected each year.

The attached sheet is a formal ballot containing those consenting individuals whose names have been placed in nomination for the present election. The information provided represents some reduction of the material sent to the Nomination Committee and encompasses advanced degree, discipline, affiliation and research interests.

Please mail ballot to:

Joan S. Lockard, Ph.D.  
Departments of Psychology and  
Neurological Surgery (RI-20)  
University of Washington  
Seattle, Washington 98195  
U.S.A.

!! BALLOT IS ON PAGE FIVE----MAIL IT TODAY !!

EXECUTIVE BOARD BALLOT

Vote for four of the nominees listed for two year terms on the Executive Board of the International Society for Human Ethology, by placing a check mark in the space provided in front of their names. Mail the ballot to Dr. Joan S. Lockard using the label provided. December 20, 1980 is the deadline for receipt of the ballot.

- Adams, Robert M., Ph.D., 1969, Psychology, University of Tennessee. Chairman, Dept. of Psychology, Eastern Kentucky University. Active in human ethology group since 1976; edited Human Ethology Abstracts III; published in the area of human ethology. Primary research interests: social behavior and family interactions in public settings.
- Burghardt, Gordon M., Ph.D., 1966, Psychology (Animal Behavior), University of Chicago. Professor, Psychology, Univ. of Tennessee, Knoxville. Research interests: ontogeny, chemoreception, predatory and social behavior in vertebrates, theoretical issues, conservation and reintroduction of primates.
- Greenberg, Mark T., Ph.D., 1978, Psychology, University of Virginia. Assistant Professor of Psychology, University of Washington. Affiliate, Child Development and Mental Retardation Center, University of Washington. Primary research interest: social behavior (e.g., mother-infant interactions) and language in normal and disfunctioning (e.g., deaf and premature) children.
- Mackey, Wade C., Ph.D., 1976, Anthropology, University of Virginia. Assistant Professor, Anthropology and Psychology, Wesleyan College. Primary research interests: cross-cultural studies (e.g., parent-offspring groupings) using age, gender, and cultural parameters as independent variables to generate predictability in human social behavior.
- Schubert, Glendon, Ph.D., 1948, Political Science, Syracuse University. Professor, Political Science, University of Hawaii at Manoa. Primary research interests: biological approach to the study of political behavior (human psychophysiology, ethology, eco-public-policy analysis, and judicial behavior (decision-making theory, psychometric, sociopsychological and socio-anthropological methods; policy analysis).
- Somit, Albert, Ph.D., 1947, Political Science, University of Chicago. President, Southern Illinois University at Carbondale. Political scientist and chairman of International Political Science Association Research Committee on Biology and Politics. Primary research interests: "biopolitics"-- the application of biological, ethological and sociobiological concepts to the study of political behavior.
- Zivin, Gail, Ph.D., 1972, Psychology, Harvard. Associate Professor, Psychiatry and Human Behavior, Thomas Jefferson University (Jan., 1981). ISHE activities: served on acting executive board, membership committee, program selection committee, and committees to propose 1982 international meeting and 1981 human ethology symposium on affective behavior. Primary research interests: processes of human communication structure via analyses of the functions and contexts of the Plus and Minus Faces during conflict and non conflict.

# Current Contents

- De Casper, A. J. & William P.F. Of human bonding: Newborns prefer their mothers' voices. *Science*, 1980, 208, no. 4448.
- Dockens, W. S. Induction/catastrophe theory: A behavioral approach to cognition in human individuals. *Behavioral Science*, 1979, 24, no. 1.
- Ekman, P., Brattesani, K.A., O'Sullivan, M., & Friesen, W.V. Does image size affect judgments of the face. *J. of Nonverbal Behavior*, 1979, 4, 57-61.
- Ekman, P. & Oster H. Facial expressions of emotion. *Ann. Rev. Psychol.*, 1979, 30, 527-54.
- Ekman, P. Friesen, W.V., O'Sullivan, M. & Scherer, K. Relative importance of face, body, and speech in judgments of personality and affect. *J. of Person. & Social Psychology* 1980, 38, 270-277.
- Feinman, S. An evolutionary theory of food sharing. *Social Science Information*. 1979, 18, no. 4/5.
- Friesen, W.V., Ekman, P. & Wallbott, H. Measuring hand movements. *J. of Nonverbal Behavior*, 1979, 4, 97-112.
- Hager, J.C. & Ekman, P. Long-distance transmission of facial affect signals. *Ethology & Sociobiology*, 1979, 1, 77-82.
- Jamieson, J.W. Genetic science, social morality and the future of mankind. *J. of Social & Political Studies*, 1979, 4, no. 2.
- Langton, J. Darwinism and the behavioral theory of sociocultural evolution: An analysis. *American Journal of Sociology*, 1979, 85, no. 2.
- Mackey, W. A cross-cultural analysis of adult-child proxemics in relation to the plowman-protector complex: A preliminary study. *Behavior Science Research*, in press.
- Pitt, J. C. Setting the parameters, or what not to say about sociobiology. *J. of Social and Biological Structures*, 1979, 2, no. 3.
- Smith, E.A. Human adaptations and energetic efficiency. *Human Ecology*. 1979, 7, no. 1
- Thiessen, D. & Barbara, G. Human assortative mating and genetic equilibrium: An evolutionary perspective. *Ethology & Sociobiology* 1980, 1, no. 2

INFORMATION ON HOW TO PREPARE COPY FOR THE NEWSLETTER IS ON  
THE LAST PAGE, WITH THE SUBSCRIPTION & MEMBERSHIP FORM.

## HUMAN ETHOLOGY

Robert A. Hinde

We all frequently acknowledge the special merits of interdisciplinary research. Why then, do we want to set up a new discipline? Why do some want to codify the term "Human Ethology"?

There are surely dangers in at least two of the points that the label "Human Ethologist" tries to make. First, it implies a group of research workers studying human behavior. Now when the biological sciences were blossoming in the nineteenth and early twentieth centuries, it was convenient to divide up the subject matter on phyletic grounds. Biologists tended to be ornithologists, arachnologists, coleopterists or what have you. Of course that was not true of the great ones - Darwin wrote about organisms ranging from fungi to man - but as the knowledge grew the natural route for specialization was along the lines of the natural classification. Gradually, however, as the growth of knowledge demanded yet further specialization, a new type of division arose. Investigators became interested in particular aspects of the groups that they studied - there were specialists in the classification of butterflies, the behavior of birds, the control of population in lemmings. Simultaneously with this, but lagging behind it, came the realization that the solution of such problems required that investigators should enlarge their scope beyond the phyletic group on which they were fixated; butterfly systematics required principles similar to those used in bird systematics; the same techniques and generalizations would serve for some aspects of insect and bird behavior; the ecology of lemmings had something in common with that of crossbills. In short, investigators became primarily systematists, comparative psychologists, or ecologists, and only secondarily specialists in a particular phyletic group.

While I am not a historian of science and so I'm not prepared to defend the precise accuracy of this account, I hope I have made my point: carving up science along phyletic lines smacks of a regression to nineteenth century science. It can of course be argued that man is special, and deserves a special focus. I agree only partially with this view, and have argued that even anthropologists and sociologists can profit by seeing some of the concepts they use put through their paces in the relatively simple case of non-human primates, and that they can gain insight into the role of institutions by comparing human societies with (relatively) institution-free non-human ones. But in any case the term human ethology comes near to being a contradiction in terms. Most of us would agree that ethology usually implies a comparative approach. Where is the comparison in "Human Ethology"?

Let us turn to a second aspect of "Human Ethology". The general view seems to be that some sort of characterization of ethology is possible in terms of subject matter, methods, and/or attitudes. Now think of where so-called human ethologists have come from. Some have escaped from the narrow constraints of Hullian theory, others from the Skinnerian mode, and yet others from the amorphous cloud of social learning theory. Some are reaching out from studies exclusively of children to see whether comparisons with other species will help. Others are finding that the experimental techniques in which they are versed need to be tempered with observation, and yet others have become conscious of biological constraints on what can be avoided at all costs? Isn't there a danger that those who follow the shiny new banner of human ethology will just shut themselves off in yet another castle? As the banner tarnishes the castle will become a prison.

Is there any answer to this? If research workers by their nature must classify themselves, how can they form groups that will not subtly impede their efforts? This is where I am glad to be able to pay tribute to John Bowlby, from whom I learned an important lesson on this issue. In the fifties I was privileged to attend a weekly meeting which included, if I remember right, two varieties of psychoanalyst, a Hullian, a Skinnerian, a Piagetian, an ethologist, some psychiatric social workers, and even an anti-psychiatrist. The discussions were immensely fertile because we were all interested in a common problem - mother-child interaction. Some may not agree with all of Bowlby's conclusions, but none can deny that his achievement has arisen in part from his willingness to accept ideas from diverse sources - even if it led to embarrassment with his own colleagues - and focus them on a problem.

And that I believe must be the answer - to focus on a problem and accept whatever material is relevant to it. The problem may of necessity be broadly defined, and what is and is not relevant may not always be easy to see. But if scientists or the products of their work are to be divided into groups, relevance to particular problems surely provides the best guide lines.

So I am suggesting that what we must do is to take from ethology what it offers to our problem, and beware of the dangers that accompany it. Let me pick out a few points on each side.

Among the characteristics of the approach of those who call themselves ethologists are

(a) Respect for the four problems of causation, development, function and evolution, instead of focussing on just the first of these. That these four types of questions are not only all of interest in their own right, but are mutually inter-fertile, is shown by Bowlby's discussion of fear and anxiety. To many psychiatrists fear of being alone was incomprehensible, for when the child was alone it seemed as though there was nothing of which to be afraid. Placed in the perspective of the forces of natural selection acting on a group-living species, the problem disappears.

(b) A comparative approach. In classical ethology this meant comparison between closely related species, usually with a view to elucidating the evolution of behavioral elements. Indeed at one time ethologists were criticized by comparative psychologists for neglecting broader phyletic comparisons, including those that involve comparisons between phyletic levels. For present purposes both can be useful. As an example of the former, van Hooff's comparative study of facial expressions in primates has thrown much light on the nature of and relations between human smiling and laughter. And light on a practical problem - schedule vs demand feeding, is provided by Blurton Jones' broad comparison between the nursing frequencies and milk compositions of a wide variety of mammals, human milk resembling that of the continuous feeders.

(c) An emphasis on observation in the natural situation as a preliminary to experimentation. I need not elaborate on this, the history of the study of child development is littered with examples.

Now let us consider a few dangers to be avoided. Our ethological approach must not lead us to make slick armchair assumptions about function or evolution. It's lovely fun to speculate about the evolutionary bases for cigarette smoking, or the relationship between the attractiveness of breasts and bottoms, but it's

hardly science. Our emphasis on beginning with observation must not lead us to disdain experimentation or physiological interference: observational techniques are only one of many tools that must be used if social behavior in all its complexity is to be understood. Because the early ethologists studied 'species characteristic' patterns, we must not seek for species characteristic patterns to the neglect of cultural or individual diversity. Because early ethologists worked with lower animals we must not be oversimplistic in our interpretations of human behaviour. The complexities of behaviour even in lower vertebrates are only now beginning to be appreciated, and we must remember that interactions between children involve affective and cognitive levels of functioning as well as behaviour. We must remember that children differ in moral and cognitive levels as well as in behavior. And that is part of what is perhaps the most important issue of all - in looking for regularities in the complexity of human behaviour, we must not be led to oversimplify.

But while I have my reservations about "Human Ethology", I am convinced by Bill Charlesworth that there is a need for a label for courses, text books and the like. However, I would argue that if we use such a label, we must be constantly aware of its dangers. I was in fact greatly inspired by a meeting of the Human Ethology Section at the Animal Behavior Society meetings in Fort Collins this summer. In searching for topics for future symposia, the section focussed on links with other disciplines. This, I am convinced, is where the future lies. Human ethology must not seek to become a self-contained, ingrown discipline. Rather human ethologists must reach out to seek for problems to which they can contribute. And when they find them, their endeavours must be accompanied by humility: there are few important problems that the human ethologist, as traditionally understood, will solve on his own.

## CONFERENCES

### Symposium Issue of Women & Politics on "Biopolitics and Gender"

Manuscripts are solicited for a special symposium issue of Women & Politics dealing with the general subject of "biopolitics and gender", although preference will be given to papers that are in principle subject to future research verification. Empirical techniques might be experimental, survey - and questionnaire-based, historical, or ethological/observational.

The term "biopolitics" connotes an emphasis on a biological perspective, which may, depending on the subject matter, be evolutionary, physiological, ethological or in some other way relevant to the life sciences. Interdisciplinary papers are thus extremely relevant.

Gender-related differences (or the lack thereof) need not be exclusively related to adult political behavior, but may deal with relevant sociopolitical behavior of persons of all ages.

Completed manuscripts should be available for a peer review process by March 1, 1981 at the latest. Submission of a final collection of manuscripts for the symposium is projected for August 1, 1981. Manuscripts should conform to the stylistic format of the journal, and be no more than 30 pages in length. Conciseness is appreciated since shorter papers will allow more topical issues to be discussed.

Persons interested in submitting a paper should contact me as soon as possible with a brief description of the project and a timetable for development of the manuscript. Please contact Dr. Meredith W. Watts, Chapman Hall 116A, University of Wisconsin-Milwaukee, Milwaukee, Wisconsin, 53201.

# \* FORUM \*

## EVOLUTIONARY BIOLOGY AND POLITICAL AUTHORITY

In what ways, if any, should ethological findings and theories affect our understanding of power and authority in human political structures? Does the prevalence of "dominance hierarchies," or "centripetal attention structures," within social groups of most higher primates suggest that there is a significant phylogenetic component in the behavioral processes which produce and perpetuate political hierarchies? Or are hierarchical gradations within political systems adequately explained by historical circumstances, ideologies, cultural norms, economic arrangements, political competition, and similar factors ordinarily considered by political scientists? Or is this an inadequate, perhaps even a misleadingly dichotomous statement of possible answers to the original questions? Are there any ways in which sociobiological theory can help us think more clearly about these questions?

All but one of the contributors to this forum are political scientists with a strong interest in evolutionary-behavioral biology, and they address some of the above questions. The non-political scientist, Professor Nishida, comments very pertinently on one of the articles listed with the original announcement of this topic.

Fred Willhoite  
Coe College

### ★ On Operationalizing "Authority"

Carol Barner-Barry  
Department of Government  
Lehigh University

One of the banes of empirical work is the need to operationalize key terms. In order to study a phenomenon, such as "authority", one must be able to identify instances where "authority" is being exercised and, conversely, to distinguish those in which "authority" is absent. In the case of "authority", the problem is compounded by the fact that "authority", is one of a cluster of related terms, such as "power", "dominance", and "influence". These terms designate phenomena, or behaviors, which are alike in some basic ways; they are, however, also widely recognized as being distinguishable. For the empirical researcher who looks to scholarly literature as a guide in operationalizing, this situation poses three basic difficulties.

First, there is a tendency for some writers to ignore the need to define terms. For example, a person writing on "authority" may simply use the term -- ostensibly assuming everyone will "know" what it means. There is no attempt to set forth that particular writer's perception of what the term means. After struggling for many years with the problem of operationalizing such terms, I find such writing baffling. Because there are so many possibilities, I am acutely aware that I am not at all sure what the author means. Also, there are frequently subtle shifts in usage during the course of a particular article or book.

Second, there are those who define one of this set of interrelated terms without relating that definition to definitions of any of the other related terms. I am guilty of that myself -- in, among others, the very article proposed as one of the four upon which this

FORUM was to be based (Barner-Barry, 1977). Although some such definitions initially seem quite clear and helpful, under close inspection or attempts at use, they tend to blur at the edges. In the particular case at hand, while "authority" or "power" might be defined, the point at which one shades over into the other is frequently quite hazy.

Finally, there are instances in which an author attempts both to write definitions for more than one of the terms and (less frequently) to relate them to each other. The problem here is that no one system has been widely adopted and, thus, can be relied upon as the implied referent when definitions are absent or only implied.

When I first began studying authority structures, I turned to political philosophy for guidance in operationalizing. After considerable digging, I emerged convinced that I was going to have to work it out myself. While very interesting and stimulating, political philosophy (at least the subset I covered) offered me very little help in solving my practical research problems.

Since that fateful (and, perhaps, misguided) decision to "go it on my own", I have found that my thinking has constantly been evolving in a continual process of adjustment and refinement as I move from the typewriter to the field and back again. Where I am at now is related to, but very different from, where I was when the 1977 article was published in Political Methodology. For the record, then, I would like to outline briefly, the picture as I see it now.

In the 1977 paper, I operationalized "authority" as follows:

An attempt at authoritative behavior was scored as successful  
 (1) if the target children accepted the initiating child's  
 decision without question or (2) if the initiating child  
 managed, physically or verbally, to persuade questioning  
 children to accept the decision without alienating the  
 questioning children. Unprovoked physical assaults,  
 harassments or takings were excluded as bullying . . . .  
 [W]hile bullying behavior might be classified as power,  
 it lacked the necessary legitimacy to be considered  
 authoritative. (pp. 426, 428)

Since the time that article was written, I have come to see both power and authority as subsets of a broader phenomenon, "influence." Because of space limitations, I will concentrate on these three, ignoring other related terms, such as "persuasion" or "dominance."

Influence has been exercised when A has been able to change the behavior of B in some significant way. Two problems (at least) immediately present themselves. First, does A's behavior have to be consciously and purposefully intended to modify B's behavior? (Wrong, 1979) At the moment, I am hesitant to require this. My reluctance stems first from the narrowing effect it would have on the behavior encompassed and the consequent impact this would have on the derivative definitions of "power" and "authority." Second, I am daunted by the practical difficulties of ascertaining "conscious intent" and "purposefulness" in field research involving children. If harder souls than I find this more tractable, however, I would welcome their suggestions.

The second problem seems, if anything, even more difficult to manage. How can you tell whether B would have done the same thing in the absence of any action by A? In the ultimate sense, this is an imponderable. In the field, one is left with a somewhat unsatisfactory solution -- the exercise of informed judgment or intuition combined (where possible) with information gleaned from one's subjects.

Moving on then, I currently limit the use of the word "power" to the subset of influence relationships which involve the use or threat of force. Power interactions, consequently, would invariably involve some element of aggressiveness or coercion on the part of A. This stipulation relates the concept of power to much of the extensive body of

ethological research focused on dominance, as well as the newer literature on attention structures characterized by the agonistic modality.

Authority seems more complex -- perhaps simply because I have worked on it longer and thought about it more. The distinguishing characteristic of the type of influence designated as "authority" seems to be the presence of some legitimatizing component. Even in the vast and diverse literature of political theory, there is substantial agreement on this point. The hitch (and there is always at least one) comes when the nature and source of that legitimacy are specified.

There seems to be a fairly broad consensus that legitimacy can be based on the occupation of a given organizational position or the possession of a certain title. Many writers seem to limit "authority" to such situations. Since my concept is broader, I would distinguish this variety of authority by appending the modifier "de jure."

De facto authority, then, would be the ability of a person to exercise influence based on personal characteristics with, perhaps, a secondary situational (but nonformal) component. In other words, it can reasonably be assumed that in certain situations persons having a given set of characteristics are more likely than others to be accepted as authoritative. It is this type of authority (i.e., de facto authority) that seems to be the basic component of "leadership" as it is treated in the most current and comprehensive political science writing on the subject (Burns, 1978).

Obviously, any person desiring to utilize this preliminary and still primitive typology as the basis for operationalization would still have many problems -- both unsolved and unindicated above. Decisions as to what behaviors should be classified under the various headings would vary from species to species, as well as among groups with significantly different characteristics within a species, such as homo sapiens.

This necessarily brief treatment was simply an attempt to introduce some of the issues involved to a potentially interested group of colleagues. Perhaps in the process, it will provoke discussion which could, in turn, aid in the development of a less crude typology. This would be useful as a basis for fieldwork and analysis, and might encourage more congruence among researchers pursuing related lines of work.

Barner-Barry, C., "An Observational Study of Authority in a Preschool Peer Group," Political Methodology, 4 (1977), 415-447.

Burns, James MacGregor, Leadership. New York: Harper & Row, publishers, 1978.

Wrong, Dennis, Power: Its Forms, Bases, and Uses. New York: Harper Colophon Books, 1979.

## ★ "ETHOPOLITICS" AND POLITICAL SCIENCE

Peter A. Corning  
Stanford University

The phylogeny (biological and cultural) of political "authority" is an important and eminently researchable problem-area. We have much to learn from ethology in this regard. But ethological approaches cannot alone elucidate the causes of this class of behaviors in humans, for multiple, interacting causes across several different levels of biological organization are almost certainly involved. The most likely methodologies, in my view, will include a combination of research tools from behavior genetics, psychophysiology, ethology, learning theory and cybernetics.

I would also like to enter some caveats about some of the pitfalls involved in making facile extrapolations from existing ethological theory and research. For one thing, recent work on "dominance hierarchies" in higher mammals suggests that this phenomenon is much more complex, multi-faceted and labile than once appeared to be the case. Second, not all ethological theories are good theories. A case in point is "attention structure theory," which, despite the early enthusiasm, is not a theory and does not explain anything. It is, at best, "diagnostic" -- an "indicator" of the cybernetic (communication and control) aspect of relationships and behavioral interactions. It cannot explain why dominance hierarchies exist or shed light on the underlying psychobiological mechanisms. Indeed, male animals frequently pay close attention to estrous females, and primate mothers are often very attentive to their offspring. Clearly, attentional patterns are a means to various ends that may have nothing to do with the classical concept of dominance. Perhaps the most striking examples are the reports of leader-follower behaviors focussed on non-dominant animals. The opportunities for studying the cybernetic aspect of animal social behavior have yet to be fully exploited, in my opinion.

The other caveat that should be stated explicitly is this: I believe there are as many opportunities for contrasts between animal and human social structures as there are suggestive analogies or, possibly, homologies. Language, lengthy processes of social learning, the existence of intricately age-graded and experience-graded organizational structures, as well as elaborate divisions of labor and responsibility, formal role-structures, formal mechanisms of leadership selection and training, etc., together create a matrix for human behavioral development and expression that is markedly different from any other animal, despite the existence of some rudimentary parallels. For example, an individual living in a complex society can, during the course of a single day, find himself/herself alternately exposed to a variety of different authority relationships and statuses as he/she interacts with, say, members of the immediate family, social acquaintances, community organizations, the church, police and government authorities and, possibly, a network of different work-role relationships. By the same token, there is no animal analogue for the human practice of systematically rotating positions of authority, examples of which can be traced from classical Athens to the contemporary American Presidency.

Political science, in short, can make good use of ethological approaches, but that is only the beginning.

#### ★ Apropos Peter A. Corning's "ETHOPOLITICS AND POLITICAL SCIENCE,"

Peter A. Corning is correct in saying that ethological approaches cannot alone elucidate the causes of (political 'authority') in humans..." I also believe him to be correct in stating that "there are as many opportunities for contrasts between animal and human social structures as there are suggestive analogies or, possibly, homologies." There might be however as many hidden pitfalls for the study of human behavior in contemporary modern human societies as in other-animaldom. Language, lengthy processes of social learning are certainly parts of a

matrix of human behavioral development in general. However, I would question whether formal role-structures, formal mechanisms of leadership selection and training, even in their rudimentary form, are as universal for human behavior. The structural-functionalists have argued, erroneously in my view, that if the function is there the structure is there too, which in turn were labelled by European anthropologists, borrowing from European experience: as states, kings, ministers, elections, etc.

Present day modern societies are laboratories of specific patterns of human behavior, but not of behavior of human beings. Corning writes that the "human practice of systematically rotating positions of authority...can be traced from classical Athens to the contemporary American Presidency." Yes, if one traces it through western history, but not if one traces it through African history. Even in some complex African societies, and there had been many of them, rotating authority was absent.

One of the fallacies of the (American) behaviorist approach is that by studying modern (American) man we study generalizable human behavior. This mistake should not be repeated. It does not mean that ethology is a better approach; it does mean that the challenge is greater than we think.

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October 14, 1980

### ★ SOCIAL BIOLOGY AND THE WELFARE STATE

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Is the evolution of human cultures subject to natural selection? Edward O. Wilson's extreme claim for sociobiology not only answers in the affirmative, but implies that all forms of cultural behavior will ultimately be found to have genetic causes. Such reductionism is impossible according to the prevailing opinion in the social sciences; for most critics of sociobiology, evolutionary theory cannot explain the diversity of human cultural practices.

Unlike either extreme, Richard Alexander presents a more complex evolutionary approach which doesn't equate natural selection with genetic determinism. As Alexander puts it, the theory merely assumes that human societies, like groups among other species, could only exist if the "specific benefits that accrue from social life" exceed the "expenses to individuals." Although based on the neo-Darwinian concept that animals seek to maximize their "inclusive fitness," social biology thus uses a cost-benefit calculus similar to rational actor models in economics, game theory, and public choice theory. As Alexander points out elsewhere, inclusive fitness theory itself involves "nothing more complex or deterministic than learning through ordinary positive and negative reinforcement schedules."

According to the prevailing interpretation, natural selection operates primarily (though not necessarily only) at the level of the individual. In social interactions between two individuals, there are four kinds of outcome, since the "actor" can derive either a net benefit (+) or a net loss (-) from an act, and the "other" can likewise derive either a net benefit or a net loss. It

follows that social situations fall into four broad categories, here called nepotism, mutual benefit, sociality, and mutual harm (Figure 1). Animals will presumably be asocial, avoiding contact with others, unless social interactions fall into one of these four classes.

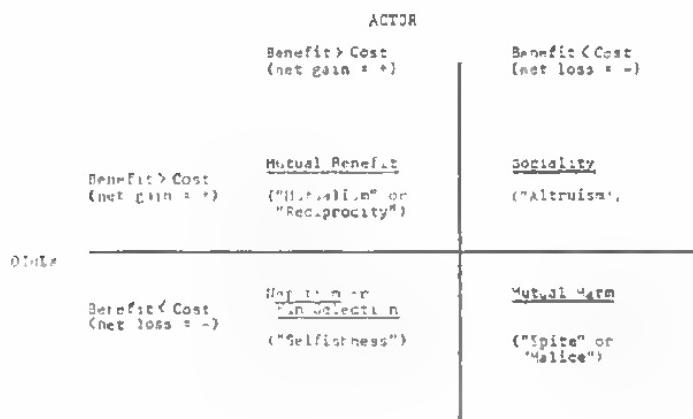
Modified from Hamilton's well-known exposition of the concept of inclusive fitness (1964), Figure 1 substitutes observable behavioral outcomes for the prevailing motivational terms (given in quotations). This shift in terminology is essential, since "altruism" may be the subjective motivation for behavior objectively classified as nepotism or kin selection (e.g., a mother dying to save her offspring). Conversely, "selfish" motives might lead to a decision not to have children, even though reducing the number of one's offspring--especially in a society facing limited resources--has "altruistic" consequences.

The modern nation-state is, in many respects, a striking case of sociality, involving social cooperation in populations of non-kin numbering in the millions. Such behavior, including a willingness to die in defense of one's country, is only possible if what Trivers calls "reciprocal altruism" is enforced by laws, police, and the apparatus of the centralized state. But such communal institutions coexist with the family (which institutionalizes nepotism) and the market economy (institutionalizing mutual benefit).

Social biology can thus be used to reconceptualize the delicate equilibrium between private self-interest (individual and family), economic exchange (market economy), and political community (nation-state). For example, individuals have a nepotistic interest in upward mobility for themselves or their offspring, whereas the reciprocity of the market economy presupposes the risk--and the reality--of downward social mobility. The modern Welfare State has become increasingly committed to protecting the standard of living of all its members, thereby minimizing downward social mobility, while trying to preserve the possibility of upward social mobility. But this objective appears to be contradictory over the long run, particularly insofar as the society ultimately encounters limits to economic growth.

Inclusive fitness theory predicts that prolonged security and abundance will reduce the benefits associated with cooperative behavior. As a result, one can argue that the modern Welfare State's success in the short run will breed the seeds of long-range disintegration. External war is one means of avoiding these internal contradictions, but it leads to situations of mutual harm (Hobbes' "war of all against all"). An evolutionary approach to complex social systems thus suggests that they may be intrinsically unstable; like all previously known civilizations, from Mesopotamia to pre-Columbian America, the modern nation-state may be fated to disintegrate--unless it gives way to a Brave New World.

FIGURE 1: Categories of Social Interaction



## ★ COST-BENEFIT ANALYSIS, SHIFTING COALITIONS, AND HUMAN EVOLUTION

By

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Coalitions are an important aspect of decision-making in small groups. A striking and recurrent feature of small group politics is the shifting of support which so often takes place and the manner in which today's majority becomes tomorrow's minority as individuals change alliances. To explain the coalition process, students of small group behavior have argued that the participants use a cost-benefit analysis to determine whether or not to become coalition members and/or whether or not to change their allegiance. Recent studies suggest that much the same explanation may also account for coalition behavior observed in chimpanzee societies.

There has long been agreement that most of the higher primate societies, our own included, are characterized by fairly clear-cut authority structures (i.e., dominance hierarchies, submission behavior, attention structures, etc.) and there has also been a general awareness that primates often use coalitions to achieve and maintain dominant status within their group. Now, going one step further, Dutch biologists report that something strikingly akin to cost-benefit "thinking" has been discerned in shifting alliance patterns among chimpanzees. If so, Pan may help us better understand the evolution of, and genetic mechanisms underlying, human behavior, a possibility made more credible by another recent article which calls attention to the remarkable similarity, in structure and organization, between human and chimpanzee chromosomes (Yunis, Sawyer, and Dunham, 1980). While some argue that other species, such as social carnivores or even some herbivores, are preferable models for explaining human evolution (See, e.g., Geist, 1978), many scholars remain convinced that the higher primates serve as appropriate subjects for cross-species comparison.

Conclusions about shifting coalitions in chimpanzees are based on observations conducted, for the past several years, on a chimpanzee troop living within a "naturalistic" zoo setting (On the justification for this, see van Hooff, 1973a, 1973b). In this group, there were three focal males--Yeroen, Luit, and Nikkie--each of which became, in turn, the alpha or dominant member of the troop. The drama began with Yeroen as alpha, Luit as beta, and Nikkie as gamma. Then, with the active support of Nikkie, Luit emerged as alpha, Yeroen fell to gamma, and Nikkie rose to beta. The next round led to further change. Yeroen now began to support Nikkie against Luit. As a consequence, Nikkie ascended to alpha, Yeroen to beta, and Luit descended to gamma (We have simplified considerably, for purposes of this brief description, the actual interactions described by van Hooff and de Waal--and especially the determined but unsuccessful efforts of each succeeding alpha to lessen the likelihood of an alliance between the then beta and gamma which could unseat him. See van Hooff, 1979; de Waal, 1978).

What might be the survival advantage of such behavior? It is generally agreed that alpha or dominant animals gain preferential access to certain valued resources. But supporters of the alpha may also reap rewards. When Luit became alpha, he permitted Yeroen (then the gamma, but one whose support was vital if Luit were to remain alpha) to copulate with receptive females. Van Hooff interpreted this as a payoff to Yeroen for supporting Luit in dominance interactions, so that siding with the alpha conferred tangible "side-payments" for his coalition partner.

Translating this into currently fashionable sociobiological jargon, we could say that backing the alpha enhances the subordinate's inclusive fitness. That is, by supporting the alpha, the subordinate increases the number of its offspring and, hence, its genes in the next generation. If the subordinate, though, decides that he will gain more by shifting allegiance to a challenger, then he will abandon the alpha. It is not too difficult to imagine some similar dynamic in human evolution (although this speculation calls for further discussion). One carryover of such a predisposition might be the propensity for shifting coalitions as individuals' cost-benefit calculus dictates in small group politics.

The chimpanzee behavior is most clearly comparable to small group behavior in humans and it would be rash to draw inferences about the biological basis for cost-benefit analysis underlying shifting coalitions at more complex levels in human society. While the phenomenon occurs in much of human politics at all levels, at all times, and in all places, it is nonetheless perilous, given limitations in the existing state of knowledge, to carry the biological argument beyond small group behavior. Even for small group politics, however, it is premature to advance any firm conclusions on the basis of cross-species comparison.

Despite the daunting problems, the issue involved--the cost-benefit analysis undergirding shifting coalitions--is important enough in the study of homo sapiens' authority relations that this evolutionary perspective demands exploration. Because this is the case, investigation of the possible evolutionary bases should be included on the research agenda of human ethology.

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★ COMMENTARY

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The field of ethology can affect our understanding of power and authority in human political structures in an important way since it has much to say about behavior, its proximate causes, its development, its ultimate function, and its history. Many of the topics of ethology such as communications, kinship behavior, mating behavior, predation, social dominance, socialization, coalition behavior, reciprocity, and territoriality are of either some direct or indirect relevance to politics. The strength of ethology as a science of behavior is that it derives its principles from evolutionary theory. So far as I know there is no other theory which has been so successful in explaining behavior, whether human or non-human.

The basic hypothesis of ethology is that behavior is molded by natural selection since it is always, to some degree, and however direct or indirect, dependent upon the interaction of genes with the environment. Those individuals whose behavior enabled them, in the environments of history, to resist the action of hostile forces such as climate, parasites, predators, diseases and mate shortages, and to do so in ways which resulted in reproductive success, would have left more copies of their genes in subsequent generations than others. The current revolution in our understanding of social behavior derives from the realization that natural selection seldom acts at levels higher than the individual (Williams, 1966) and that what matters is not only the individual's own reproduction but also that of his relatives, to the extent that his assistance might have enhanced it (Hamilton, 1964). If this theory is correct, it suggests that political behavior will be an historical consequence, under varying environmental conditions, of the propensities of individuals, singly or jointly, to behave in ways that are reproductively selfish.

The relevance of evolutionary theory (equated by some with "biology") for understanding phenomena such as politics (equated by some as "culture," and hence, non-biological) is often doubted by political scientists. Most biologists would reject this sharp dichotomy and try to understand the evolutionary significance of culture. Their basic hypothesis is that much of culture has in fact been adaptive in the evolutionary sense and has enhanced the inclusive fitnesses of the individuals who have used, maintained, changed, and transmitted it. If this were not so, individuals ("genetic replicators") who were capable of using, maintaining, changing, and transmitting culture would have disappeared along with their culture, as a consequence of natural selection. A major accomplishment of the Chagnon and Irons volume is to demonstrate that many otherwise confusing cultural phenomena do make sense

Over the last 10,000 years or so a remarkable transition has occurred from a world in which small, hunting-collecting societies prevailed to a world dominated by nation-states. From a situation in which leadership was ephemeral, political authority weak, hierarchy virtually absent, and social exploitation minimal, we have reached a situation that is just the opposite. How did these changes, which were largely cultural, get started? What caused them? What was it in the evolutionary history of humans (or their ancestors) that preadapted them for such changes? It is probably here that evolutionary theory can make a unique contribution to an understanding of what produces and perpetuates political hierarchies.

Anthropologists know that about 99% of human cultural life was spent in hunting-collecting societies. To understand this stage of cultural history they have tried to identify the selective forces which were important in hominid evolution, to learn why the hominid line speciated, and why only the line leading to humans survived. The issues here are still controversial. The most likely story, however, is that the line leading to humans acquired a multi-male band structure which afforded protection against the predation of large carnivores and also allowed the hunting of large prey. This line, when it came into competition with other hominids, killed them off.

Anthropologists' understanding of this stage also relies, to some extent, upon the comparative method that Darwin so successfully used. Comparisons of the morphology and behavior of humans with those of higher primates do show many similarities. Anyone who observes the dominance behaviors of the chimpanzee will probably recognize instantly some similarities to human behaviors (e.g., glares, hair-erection, screams), while other behaviors will seem unfamiliar (e.g., barks, pant-grunts, supplanting). We are probably safe in saying, then, that at least some dominance behaviors of humans owe their origin to kinship with other higher primates. Of course, this does not mean that any of these are of much importance in politics. There are several behaviors that we share with higher primates, however, that are of particular interest because of their obvious relevance to politics: age-graded dominance hierarchies and coalition behavior. Knowing more about the phylogeny of these behaviors, as well as the reasons for their extraordinary expressions in humans, including the function of language which makes politics possible in the first place, would contribute greatly to our understanding of political behavior.

Raymond Dart was the first to notice that the social behavior of humans may be more closely aligned with that of the carnivora, as a result of convergence, than with the primates. Thompson (1974) substantiated Dart's argument. For some time anthropologists have studied the social behavior of group hunters such as wild dogs, hyenas, and wolves. With regard to some behaviors, such as social reciprocity (e.g., food sharing), patterns in the group hunters do more nearly resemble the primitive human condition. Reciprocity is another behavior that is greatly elaborated in humans, and knowledge of the reasons for this should also contribute to our understanding of political behavior.

Perhaps the major question in human evolution is the cause (or causes) of traits, both non-cultural and cultural, that are unique or distinctively expressed in humans. Alexander and Noonan (1979) have listed two dozen of these and argue that there may be a single cause for the emergence of all of them: "The attribute that could cause differential reproduction leading to human uniqueness, we believe, is an increasing prominence of direct inter-group competition, leading to an overriding significance in balances of power among competing social groups, in which social cooperativeness and eventually culture became the chief vehicle of competition" (pp. 439-440). If this is so, and politics more complex than those of hunting-collecting societies are the consequence of inter-group competition operating through such mechanisms as

alliance formation and conquest, an answer to the question of what produces and perpetuates political hierarchies becomes somewhat clearer.

Evolutionary theorists reject the idea that there is any "intrinsic" benefit to group living--there is no mysterious economic, social or political glue that holds societies, and especially complex polities, together. Indeed, there are inevitable costs to group living such as increased susceptibility to parasites and diseases and increased competition with other individuals for mates and resources. The benefit that humans do derive from group living is probably extrinsic--the protection that is obtained from hostile conspecifics in other polities. The field studies of the Gombe chimpanzees are fascinating in this respect because they indicate that this factor may also be important in the social grouping of this species.

If there is nothing holding complex polities together, other than the danger posed by other hostile polities, we would expect that polities would fission or break up more frequently whenever this danger has diminished. A number of anthropologists have already noted the nearly universal tendency of less complex polities, such as segmentary lineages and chiefdoms, to fission once they grow larger, and presumably more powerful and secure. Fissioning tends to reduce the average size and complexity of polities.

Fissioning, however, is risky. Chagnon's studies of this process among the Yanomamo seem to show the dangers to small polities trying to go it alone when they are vulnerable to attack by larger and more powerful polities. More generally, warfare has been a potent force reducing the number of polities over most of human history. Those polities have persisted that have waged war, whether defensively or offensively, in an effective fashion. For this reason I would expect that the political structures existing within polities that are fully autonomous will reflect to an important extent the functional test of waging war effectively. They are the survivors.

For 99% of human cultural life the size of polities was limited by the conditions of subsistence. These conditions were largely eliminated with the advent of pastoralism and agriculture. As some polities became larger, as a result of population growth, immigration, conquest, or some other reason, this would shatter the prevailing equilibrium. The nature and intensity of balance-of-power races would change. The structure of polities would correlate even more closely with prevailing military tactics, technology, and organization.

What enables hierarchical structures, and associated patterns of exploitation, to persist? Presumably a necessary condition is the continuous presence of hostile polities. Otherwise, as exploitation increased, individuals and groups would leave, and polities would fission. Chagnon's study of the Yanomamo would seem to support this point. I doubt that the Yanomamo elite (I would not label the Yanomamo an egalitarian society) would be so successful in exploiting their positions for personal and family advantage if it were not for the perpetual state of danger that their polities face. I think the same would be true of modern nation-states. In Yanomamo culture the egotistical, sexual, and nepotistic excesses of elites have the effect of enhancing their own inclusive fitnesses. This may or may not be so in the cultures of modern nation-states. Whatever the case, these excesses do have consequences for polities. Dynasties are built and destroyed. Political elites rationalize existing patterns of exploitation, if they are conscious of these, and if it is necessary to do so, by emphasizing their supposedly indispensable role as protectors. Ideology, nationalism, and religion are often useful in these efforts to retain legitimacy.

Although the presence of hostile polities may facilitate the persistence of hierarchy and exploitation, it does not seem to insure it. The tactics, technology, and organization of effective warfare may change in ways that are . . . . . maintaining networks of hierarchy and exploitation supposedly.

polities will either adjust to these changes or will perish. Also, the assymetries in power and perquisites associated with extreme hierarchy always attract the envy of individuals and groups who would have more of these things. Sometimes these out-groups have the resources and skills for successful challenges of incumbent elites, who are then either replaced or forced to share power. The resulting structural changes which occur to polities may or may not be compatible with effective warfare.

On these points an account informed by evolutionary theory does not seem especially novel and would probably differ little from that of the historian, sociologist, or political scientist. The evolutionary theorist will look at many of the same variables but with a somewhat different focus, a renewed attention to the causal priority of particular variables, and confident expectation of the importance of evolution to human history. All of the articles seem to be steps in this direction.

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#### ★ ON INTER-UNIT-GROUP AGGRESSION AND INTRA-GROUP CANNIBALISM AMONG WILD CHIMPANZEES

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As early as 1968 I pointed out that chimpanzee unit-groups or "communities" (mentioned hereafter as Group) are antagonistic to each other and that dominance-subordination relationships influence the movement of groups (Nishida 1968). But my paper was mostly neglected by Western primatologists and anthropologists. In 1972, we published a more detailed report on the inter-group relationships between two habituated groups (Nishida & Kawanaka 1972). Though this article included two focal points, namely inter-group antagonism and inter-group female transfer, only the latter topic received attention, probably because wild chimpanzees had been pictured as very peaceful creatures by pioneer workers, and partly because of the unconscious belief that man's nearest relatives should be "as peaceful as (??) human beings."

Though I am not amazed to hear that groups at Gombe are also antagonistic, I must confess that it was far beyond my expectation for chimpanzees of the bigger group deliberately to search for and kill chimpanzees of the branch group. In this essay I am willing to explore biological bases of such murderous events.

Most chimpanzee females move out of their natal group and transfer permanently to one of the neighbouring groups when they reach sexual maturity (Nishida 1979). Wrangham's (1979) thesis that each female has her own individual territory is rather misleading. When females transfer, they finally change their moving range completely and also their associating conspecifics. No close three-generational matrilineal subgroup has ever been observed in our study groups. The fact that the Flo-Fifi group has been Goodall's major observational target seems to have biased our understanding of mother-daughter (and female-female) relationships of wild chimpanzees. In fact, such a matrilineal group (mother and her adult daughter) seems rather exceptional.

On the other hand, adult males have been observed to enter study groups neither at Gombe nor at Mahale (van Lawick-Goodall 1973, Nishida 1979). It follows that males remain in their natal group and that females move out of it, except in such cases as a cycling female transfers, accompanied by her juvenile son. Therefore, males generally are more genetically related to each other than females are to each other. I have pointed out that males of a group are much more strongly bonded with one another than females are with one another (Nishida 1968, 1970). This may be based on co-socialization, which favors closer bonds among relatives. The function of male bonding might lie in the common benefit of communal defense of the territory (Wrangham 1977).

Why then did the males exterminate their old friends? In spite of the above generalization, I think that there are closely related males and distantly related males in one and the same group. If an adult male's mother originates from Group A and another male's mother from Group B, and their fathers are different, it is expected that their relatedness is very remote and that the opportunity of their co-socialization decreases. Emigrant females from the same group show a strong tendency to stay in close proximity to one another if they transfer to the same neighboring group (Nishida & Kawanaka 1970, and unpublished data). One might speculate that killer males were more genetically related to each other than they were to victim males. This probably structured the original fission of the study group and occasioned the subsequent inter-group murder.

The episode, however, cannot be fully understood without ecological considerations. Gombe National Park is surrounded by Lake Tanganyika to the west and by human settlements, shambas, and roads in the other direction. Human populations living in the periphery of the Park have increased recently, and most of the riverine forests outside the Park have been completely destroyed (personal observation). Habituated chimpanzees occasionally moved out of the Park in the past (Goodall et al, 1979). The carrying capacity of the territory of the study group probably had decreased recently. Probably this devaluation of the habitat pressed on the habituated chimpanzees, as Goodall et al. pointed out.

Another aspect of the murder is the size of competing groups. The fact that the murderer group is much bigger than the victim group lowered the cost of such dangerous practices. At Mahale, males of the smaller study group (K-group, originally consisting of 5 adults males and one adolescent male in 1967) disappeared one by one (one in 1969, one in 1970, two in 1975, one in 1978 and one in 1979). Though one male was suspected dead and another was ostracized by a younger male, the other four males were prime adult or young adult males. No symptom of loneliness or disease was observed on the part of the missing males before they disappeared. Moreover, we could not find them in the periphery of the K-group range, nor in the range of two neighboring groups. It is plausible that at least some of these males were killed by chimpanzees of the dominant M-group that consisted of 16 adult males in 1974 and that occasionally invaded even the core area of the K-group. M-group males were suspected of killing an infant of the K-group within the core area of the latter group in 1976 (Nishida et al., 1979).

Another topic might be more sensational and puzzling. A particular mother-daughter dyad killed and ate infants of mothers of the same group. In the K-group, most of the young nulliparous females who joined from outside have been observed habitually to take care of infants (0.5 - 2.5 years old) of non-related primiparous, or parous mothers who had no other elder offspring in the group (Nishida 1979). But, no cannibalistic episode has been observed. This is probably because the mothers were always highly dominant over the immigrant nulliparous females (See, Hrdy 1979). Therefore, it is very costly to attack the infant, and moreover play-mothering may be beneficial for babysitters who practice maternal care (Lancaster 1971) and also for mothers who receive not only grooming services from the babysitters themselves (Nishida 1979) but also obtain leisure time spent in social grooming with other companions.

Why then female cannibalism? This is not astonishing genetically, because, as already mentioned, females of a group generally are distantly related and compete for resources. But it is unlikely that these are frequently observed phenomena, since the cost of killing is high. Probably males of the group (mates of the mother of the victim infant) are expected to respond to such practices by attacking the cannibal female. If female cannibalism within a group is a common practice, female transfer would not have evolved, since genes predisposing inter-group transfer would be rapidly eliminated. Needless to say, immigrant primiparous mothers are most vulnerable to such an attack. Parous females have scarcely been observed to take care of infants of other mothers, even if they lost their own infants. This might be mainly because they need not practice maternal lessons, but it is possible that primiparous mothers may refuse to deliver their infants to parous mothers who are more dominant and might abuse them.

I predict that cannibal females are dominant, parous females who are only distantly related to mothers of victim infants, if such episode is ever seen again. But this is only the necessary condition of the female cannibalism. What is the sufficient condition of the episode? I do not hit upon any idea except that this might be a rare psycho-pathological case.

Finally, I would like to add another two cases of cannibalistic episodes observed recently at Mahale. Norikoshi (1979) and Kawanaka (unpublished) observed M-group males kill and eat very young infants of immigrant females from K-group in 1977 and 1979 respectively. The father of the infants is surely one of the M-group males, at least in the former case. As I already pointed out, there are closely related males and distantly related males in the same group. It is suggestive that the killer male was not the alpha male, who was the most probable candidate for the victim's father.

At any rate, it has become apparent that chimpanzee mothers live under very dangerous social situations. We must observe subtle relationships between mother-infants and their conspecifics very carefully. Moreover, we must reconsider "safari behaviour" of a particular dyad of a male and a cycling female (Tutin 1979) from a viewpoint of a strategy to counter the menace of conspecifics to their offspring.

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